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COMMENTARY

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Response to: Comment on "Peatland carbon stocks and burn history: Blanket bog peat core evidence highlights charcoal impacts on peat physical properties and long-term carbon storage" by Evans et al. (*Geo: Geography and Environment* 2019; e00075)

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[Correction added on 10 July 2019 after first publication: the year in the citation 'Evans et al.' in the first paragraph has been corrected in this version.]

We would like to thank the authors Evans et al. (2019) for submitting a comment on our recent publication "Peatland carbon stocks and burn history: Blanket bog peat core evidence highlights charcoal impacts on peat physical properties and long-term carbon storage" (Heinemeyer et al., 2019); we especially value their direct and open approach. We hope that our response clarifies our methods, findings and conclusions; we also provide further references and more detailed information around the limitations and remaining knowledge gaps.

We do understand that burning on peatlands is a highly controversial issue, not just in the UK (i.e., grouse moor management on deep peat/blanket bog) but also globally, particularly in the tropics (e.g., agricultural management on deforested and drained peatlands). We therefore would like to clarify up front that our findings are to be seen only in the context of rotational burning on UK upland blanket bogs – an interpretation within other fire contexts, specifically a tropical context, is not and never was felt appropriate by the authors. We previously clarified this limitation in our conclusions: "… estimates are based on low severity prescribed burns and the impacts of more severe arson or wildfire are likely to differ (i.e., when peat burning occurs)." However, within the UK context we feel this work adds considerable weight to the somewhat limited, but now growing body of evidence regarding prescribed heather burning impacts on blanket bog ecosystem services, specifically carbon storage. We would also like to clarify that our previous and additional criticism of Garnett et al. (2000) is exclusively based on the data presented. Indeed, as the only major study on prescribed burning impacts on soil carbon stocks at that time (as highlighted by Evans et al., 2014), we feel that Garnett et al. (2000) should be subject to detailed scrutiny in order to determine gaps in our understanding and inform future research. While we appreciate that the comments made by Evans et al. (2019) are well intended, we remain confident in the robustness of our data and below we defend the methods and main results. For this, we provide further clarification and justification of our methods, together with providing some additional references and graphical information to support the interpretation of our findings and our overall conclusions.

Evans et al. stated that our findings “could be net beneficial for C sequestration” and that this is “contrary to most current understanding.” Firstly, we do not make such claims as we did not include an unburnt comparison; we clarified this previously in the conclusions of our published paper: “Finally, our results do not allow a comparison to an unburnt scenario and estimates are based on low severity prescribed burns …” Secondly, the Evans et al. statement “most current understanding” is not backed up by any references and Evans et al. (2014) even highlight that the evidence base is noticeably weak, which has been confirmed by a recent review by Harper et al. (2018). In fact, Evans et al. (2014) help to demonstrate this point by only using one long-term study (Garnett et al., 2000) to model the relationship between burning and C storage in UK blanket bogs. Thirdly, “current understanding” may be incorrect because, in addition to our study, several new studies (as identified by the authors) have weakened the unsubstantiated claim that prescribed burning greatly reduces carbon accumulation, particularly if considering management and monitoring timescales (e.g., Clay et al., 2010; Marrs et al., 2019). Notably, a recent study by Marrs et al. (2019) used the same plots as Garnett et al. (2000) but employed more detailed lead isotope peat depth dating. Importantly, they found that prescribed burning only caused significant reductions in peat and C accumulation rates within the most intensive 10-year burning treatment; however, carbon and peat were still accumulating at a considerable rate (Marrs et al., 2019). However, Marrs et al. (2019) point out the likely relevance of bulk density (but unfortunately no bulk density data are shown). Moreover, charcoal has been highlighted as an important but so far overlooked factor in explaining high carbon sequestration in northern peatlands (Leifeld et al., 2017) and UK blanket bogs under heather burn management (Clay & Worrall, 2011). Therefore, our previous findings about charcoal impacts on bulk density and thus C accumulation are of key importance in interpreting these latest data from the Hard Hill burn plots. Interestingly, a PhD student, co-supervised by Evans et al., presented a poster showing C accumulation rates based on similar spheroidal carbonaceous particle (SCP) dating to be greatest for burnt plots across different management on blanket bog (Collier et al., 2016). Moreover, we suggest that to determine any meaningful net impacts on C sequestration, a catchment-scale approach needs to be considered, specifically including topography (i.e., slope) and runoff (i.e., erosion). However, this is largely lacking from the evidence base. The authors also state that “these findings could have significant consequences for land-management policy.” We do not dispute this, but feel that this is exactly what research should aim for, particularly where the evidence base is weak or controversial, as is the case for prescribed burning impacts on blanket bog habitats in the UK (Evans et al., 2014; Harper et al., 2018). Notably, further evidence (which so far seems to be overlooked from the evidence base alongside Clement's (2005) PhD thesis) is provided in a PhD thesis by Grand-Clement (2008), who used lead isotope dating and found that unburnt cores showed only half the peat accumulation rates of burnt cores (cf. Chapter 8: pp. 160–161 and 180–184, although the study acknowledged lead isotope dating uncertainties). However, we never stated that our findings or conclusions applied to areas outside the UK (as implied by Evans et al.) – see above for quotations from our previous publication. Our paper has a clear UK focus because we are more than aware of the environmental heterogeneity of peatlands across the globe (e.g., vegetation type affecting peat bulk density and hydraulic conductivity, as well as management and climate factors potentially affecting peat decomposition).

Firstly, Evans et al. criticise our failure to include an unburnt control. However, this is not justified because our hypotheses clearly do not require one (i.e., we were looking at the relationship between burning and C accumulation and bulk density; we were not comparing burning to non-intervention). We also highlight within the paper that, ideally, future research should include such a comparison – although this has now been done by Marrs et al. (2019); nevertheless, Marrs et al. (2019) did not measure C_{org} directly and did not report bulk density values (which we show are both crucial for C stock determination and are required at a very detailed and continuous depth increment resolution). Furthermore, our annual C accumulation data (transformed into t CO_2 per hectare) since the 1950s (ca. 3 t CO_2 ha^{-1} $year^{-1}$) fall right in between (albeit they are not directly comparable as time frames are slightly different) the rates reported for unburnt plots by both Garnett et al. (2000; ca. 3.8 t CO_2 ha^{-1} $year^{-1}$) and also Marrs et al. (2019; 1.7 t CO_2 ha^{-1} $year^{-1}$) from the 1960s onwards. Evans et al. highlight the rather small differences in the overall burn frequencies (since 1,700) between our sites (23, 25, 28 years). But in doing so, they ignore the more distinct and regular burn frequencies (13, 16, 22 years) within the more recent period (1950–2015) shown in our original table 1 (Heinemeyer et al., 2018), which are in fact very similar to the 10–15 years of anticipated current grouse moor burn frequencies. Therefore, there are important and representative present-day differences in burning frequencies between our study sites. Moreover, Evans et al. criticise the lack of more within site sampling, yet sampling across a wider area with climatic differences should be seen as an advantage, as it offers real and meaningful replication rather than providing detailed records for only one site. Importantly, we do find a very similar positive relationship (and similar changes with depth or age) between bulk density and C_{org} versus charcoal amounts at all three sites. This implies that the findings have general implications and are less dependent on local climatic conditions or differences in land-use history. Furthermore, our sites represent the characteristic range of UK upland grouse moor conditions (wetter and colder at Mossdale to drier and warmer at Nidderdale), which should be seen as a methodological strength rather than a weakness. Finally, the three peat cores were taken within each site to allow multiple analyses. To treat them as independent replicates would be misleading as, at this distance, they would clearly represent pseudo-replication; any duplicate data (e.g., charcoal counts) were therefore pooled. The related Evans et al. statement that multiple cores taken over larger areas would show greater within-treatment variability is, of course, to be expected but not surprising in ecological soil work. However, for our hypothesis, additional within site sampling was not required as replication is provided by comparing data and relationships across the three sites. Again, we highlighted that further samples across the entire catchment (i.e., slope areas) and other sites should address such issues affecting C accumulation. Specifically, we would expect considerable erosion losses (and thus a negative C balance) from burning on steep slopes due to possibly increased runoff (Clay et al., 2009) and decreased vegetation leading to increases in overland flow (Holden et al., 2008); however, to our knowledge only general, rather than specific (i.e., studies accounting for different environmental conditions), peat C accumulation modelling studies exist in this respect (e.g., Heinemeyer et al., 2010).

Secondly, regarding the dating of the lowest peat depth (25 cm), Evans et al. claim that this was done "without supporting evidence." However, not only did we already acknowledge within the paper that this age is uncertain, but we also provided two references for the estimated 1700 age; based on the very similar C accumulation rates to the Garnett et al. (2000) unburnt plots, this assumption is a valid, albeit uncertain assumption. Notwithstanding this uncertainty in the lowest peat age (for which no SCPs could be used), the main focus of this study is on the top peat layers for which SCP dating was possible; it is these layers which revealed a strong correlation for both bulk density and C_{org} versus charcoal amounts. Moreover, the criticism of the Garnett et al. (2000) arbitrary SCP "take-off" age determination is still important, particularly as it is the only study included by Evans et al. (2014) to model prescribed burning impacts on peat C stocks. We are not questioning the credibility of Mark Garnett's studies; but rather, we only query the interpretation of their presented SCP data, and the age determination, which we believe are very likely flawed. This claim is supported by more recent assessments of the Hard Hill burn plots, which either use carbon stock (Marrs et al., 2019) or flux (Clay et al., 2010) techniques. The huge reductions in C accumulation on burnt compared with unburnt plots reported by Garnett et al. (2000) do not seem to relate to any recent studies at the same site, which show a small C loss or even potential C gains in response to prescribed burning. Furthermore, we believe that it is difficult to interpret the SCP and charcoal depth profiles of Garnett et al. (2000) correctly (refer to their figures 2 and 3, respectively) because not all plots are shown (only for two blocks but all seem to be used as independent replicates in the ANOVA statistics) yet no reason is given; and those plots that are shown reveal no expected charcoal layers (i.e., while clear charcoal peaks are shown for unburnt plots, profiles for burnt plots do not show such expected charcoal peaks) nor do they resemble an expected SCP profile (for burnt plots there are no SCP peaks at all and there is hardly any SCP increase until the most recent periods). Evans et al. question our "noisy SCP data," but we question the SCP data shown in Garnett et al. (2000) because they completely lack any SCP peaks for burnt plots. Moreover, as Evans et al. claim, burning may increase SCP concentrations via combustion of the peat layer (e.g., leaving SCPs behind), which should result in an obvious and strong SCP signal in burnt plots (but it is not; see Garnett et al.'s figure 2). Even excluding analysis in the near surface layers should have shown a clear SCP peak (in the 1970s) and clear charcoal peaks for burnt plots (indicating regular burn events over time). Also, the claim made by Evans et al. that SCP "take-off" is more robust than SCP peaks or onset is not backed up by any reference. In fact, a PhD thesis by Clement (2005) used the same method as Garnett et al. (2000) but questioned this "take-off" approach. Clement (2005) points out uncertainties of such a "take-off" assumption and specifically questions Garnett et al.'s 1950 "take-off" date by highlighting that it more likely reflects the 1850s, which was confirmed by comparing corresponding C accumulation rates. Importantly, both studies base their SCP dating on only 1 cm peat sections. Thus, our 0.5 cm increments should provide a more robust SCP count, peak, and peat depth/age determination. The fact that the Garnett et al. (2000) study failed to detect a clear charcoal signal from burnt plots means that it is extremely difficult to determine the onset of the burn rotation (actually, the top left unburnt A/G graph, cf. figure 3 in Garnett et al. (2000), looks like a burnt plot with clear charcoal peaks, even when considering the different axis scale). There are some additional abnormalities with the charcoal graphs presented by Garnett et al. (2000) (cf. figure 3). For example, while one burnt (GB) plot assessment only goes to 9.5 cm, the other goes to 18 cm (a huge difference); importantly, the latter clearly indicates a potential error in the 1954 burn age (i.e., there are two lower charcoal layers, one likely 1954, the other 1923; if the age were moved then the burnt and unburnt depth would be very similar at about 13 cm for 1954 and 17 cm for 1923). Unfortunately, interpretation remains limited as Garnett et al. (2000) did not present graphs for all plots and/or graphs with matching depth profiles. Evans et al. also suggested that we should have consulted Garnett and Stevenson (2004) before criticising Garnett et al. (2000). However, we feel the study is not directly relevant (this is why we did not cite it) because the study only looked at ^{14}C ages for two unburnt plots (comparing a burnt and unburnt plot would have been more helpful). Also, the ^{14}C ages shown are very noisy (and not in "high agreement" as suggested by Evans et al.; see their figure 1 for % modern ^{14}C data) and therefore are unhelpful in age determination; the authors (Garnett & Stevenson) even acknowledge this by stating that, despite the fact that ^{14}C dates "were in broad agreement," "there were uncertainties in the final interpretation of the ^{14}C results" and ^{14}C may have been contaminated by modern root-derived C inputs. While there is no doubt that a fire occurred in 1923, we remain of the opinion that the incomplete (both total number and similar depth) charcoal graphs in Garnett et al. (2000) and the lack in any subsequent ^{14}C ages for only two profiles prevent a robust age determination of the charcoal layers.

We acknowledge that the SCP "take-off" method is used in many studies, however Clement (2005) showed there are considerable uncertainties around using such values. We provided all the relevant information regarding SCP dating. However, we now acknowledge that we omitted to provide our justification for choosing a peak age selection of 1975. Firstly, Swindles (2010) identified the peak as 1979 but with a considerable tendency towards a younger age (i.e., the shape of the curve is flatter towards a younger age). Secondly, Swindles et al. (2015) state a peak age of 1977 ± 5 for a blanket bog at Malham Tarn in the Yorkshire Dales, which is very close to all three of our peatland sites. We therefore chose a more conservative age of 1975 as our peak age. Importantly, a more recent date is also supported by a publication (poster) using 1976 as the SCP peak with Evans listed as a co-author (Collier et al., 2016). We have now added this information to Figure 1 (an enlarged section of the top SCP peak area for all three sites). Figure 1 clearly shows that the peak shape is very similar for all three sites. Although there is a "multiple-sample peak" (but in figure 3 and not in figure 1 shown in Heinemeyer et al., 2018, as stated by Evans et al.), all three sites reveal the same pattern of a smaller peak either side of the main peak (see Figure 1, which is an extract of figure 3 in Heinemeyer et al., 2018). Importantly, the difference between 1975 and 1979 is less than 0.5 cm of peat and would be similar for all three peat cores. Therefore, the selection of 1975 as the peak age, which sits comfortably with the 1977 ± 5 estimate given by Swindles et al. (2015), probably had very little impact on the findings of our study. We would certainly assert that our SCP dating is more robust than that of Garnett et al., which does show "very noisy" SCP data and without any clear SCP peaks overall. However, we agree that our study would have benefited from additional dating methods, which we stated within the paper. However, other dating tools such as ^{14}C, as pointed out by Evans et al., also have considerable caveats (see our above comment on Garnett & Stevenson, 2004). Clement (2005) showed that robust dating is ideally required from a number of other sources, with ^{210}Pb, and dating from atomic weapons testing (e.g., ^{137}Cs and ^{241}Am), playing a key role in confirming chronologies based on an indirect method such as SCPs. Furthermore, we do not feel that acrotelm growth, decomposition, or peat combustion influenced our results. As our sites are subject to a burn rotation, effectively preventing build-up of combustible vegetation (see Davies et al., 2016), there is little chance for any considerable peat combustion, particularly as "cool burning" is practiced during late autumn/winter. In fact, if there were any such events we should have found visually clear charcoal layers (horizontal bands) containing considerable charcoal-peat fragments, which we did not observe. Therefore, our charcoal fragments largely represent charred vegetation remains. Nevertheless, the effects of acrotelm growth, decomposition or peat combustion are equally likely to have influenced the Garnett et al. (2000) plot-level SCP analysis. Surprisingly, the burnt plots in Garnett et al. do not indicate any considerable charcoal peaks (charcoal counts are mostly lower compared with all the unburnt plots shown, even when considering the different scale in their figure 3) nor higher SCP levels (SCP levels of all burnt plots are remarkably low until near the top peat surface). In fact, several unburnt plots (but none of the burnt plots) indicate clear charcoal peaks and also higher but very noisy, low, and uncharacteristic (i.e., no peak) SCP levels (cf. SCP counts shown in Swindles et al., 2015). We have since tested for the relevance of any such combustion impact on SCP levels. If there were any SCP concentration from peat combustion, then one would expect to find a very strong correlation between SCP and charcoal concentrations (over all measured and corresponding 0.5 cm layers). However, for our three sites this (linear regression) analysis (Figure 2) did not reveal any meaningful pattern. In fact, for Nidderdale there was no significant fit observed, while those for Mossdale and Whitendale were very noisy (low R^2) and only weakly significant (very low p-value). Finally, none of our charcoal concentrations indicate severe burns (mostly concentrations of around 50–200 per cm^3, with a few values of up to 2,340 per cm^3 and mean ± standard deviation for Mossdale, Nidderdale, and Whitendale: 66 ± 77, 258 ± 475, and 373 ± 468 per cm^3, respectively), and are very similar to the concentrations reported in figure 3 by Garnett et al. (2000) when combining all size classes. However, we do not question the potential for such a SCP concentration process from burning peat, which is feasible particularly when past wildfires burnt into the peat, so other studies should ideally consider and test for this potential artefact. We also think that the SCP criticism is unwarranted due to clearly similar and robust peak patterns recorded across all three sites (Figure 1) (unlike for the SCP data shown in Garnett et al., 2000), and a lack of possible peat combustion affecting SCP concentrations (Figure 2). In fact, a comparable SCP analysis for a peatland on the Moor House site (near the Hard Hill plots) as part of a PhD thesis by Clement (2005) also noticed unusual but similar SCP multi-peaks (cf. figure 5.5) as observed in our study (albeit with a different overall SCP abundance). Importantly, while our SCP counts are higher compared with those reported previously, they fall within the expected range as outlined by Clement (2005), who also shows that our three sites are located in an area of very high SCP deposition (cf. figure 5.10). Unfortunately, no SCP concentrations but only counts (and no indication of volume or dry mass corrections) are given by Garnett et al. (2000), which prevent us from directly comparing SCP values (which we consider to be very low in the Garnett et al. study compared with, e.g., Swindles et al., 2015).

Evans et al. state that we concluded that more frequent burning since the 1950s has increased C accumulation rates. This is not what we intended. Our comparison is between the three sites in relation to burn frequencies determined by charcoal peaks within each of three time periods (previous figure 6 versus table 1 in Heinemeyer et al., 2018; comparing like with like). Measurements (see our previous table 4) and modelling (Heinemeyer et al., 2010) clearly indicate that C

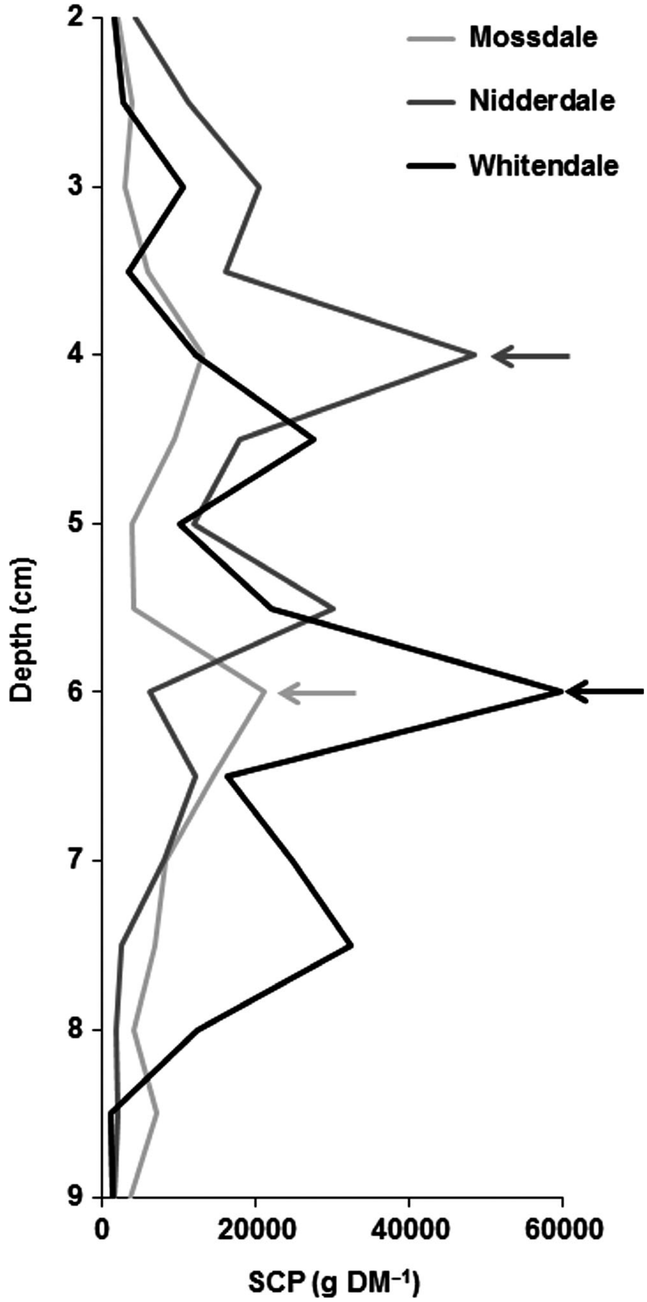

FIGURE 1 Peat core depth profile for spheroidal carbonaceous particle (SCP) counts (per gram dry matter [DM] mass over the detailed top section around the SCP peak) for the three sites determined in 0.5 cm sections between 2 and 9 cm (note the total SCP profile from 0.5 to 15 cm depth is shown in Heinemeyer et al., 2018), with arrows indicating the peak SCP counts corresponding to the conservative estimated year 1975 (a conservative estimate based on Swindles [2010] stating 1979 and Swindles et al. [2015] defining it as 1977 ± 5).

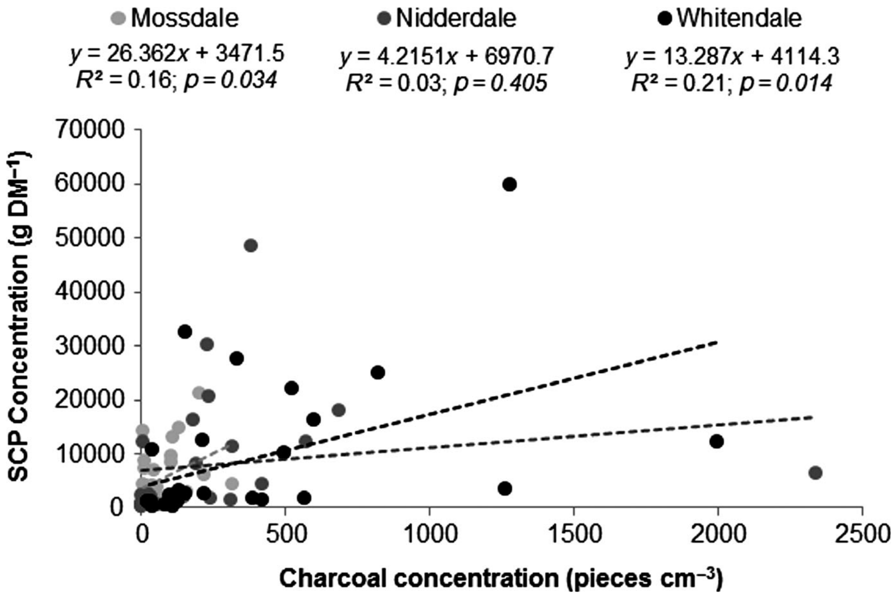

FIGURE 2 Spheroidal carbonaceous particle (SCP) counts per dry matter (DM) mass versus charcoal concentrations per intact (wet) peat volume (with a size fraction of >120 μm) through the peat core depth profile for the three blanket bog sites (cf. Heinemeyer et al., 2018), determined for each 0.5 cm section to a depth of ca. 15 cm (i.e., up to the depth where SCPs were detected; n = 29, 25, and 28 for Mossdale, Nidderdale, and Whitendale, respectively). Linear regression lines (best fit equation and R^2 values) are shown together with the p-values (Excel regression analysis) for the three sites based on Excel line fits.

accumulation rates are much higher towards the peat surface due to the "acrotelm effect" (simply as peat has not yet been decomposed for long enough). Therefore, our main analysis was not over time but between discrete time periods across sites (which differed in C accumulation rates in relation to burn frequencies). The main point in our analysis is that, due to charcoal input from burning, the resulting C accumulation rates are positively affected by increased bulk density and C_{org} (similar regressions for each site per period or peat depth).

Finally, Evans et al. point out "that the results of the peat core study appear to directly contradict chamber-based CO_2 flux measurements." We clearly noted in our paper that there is a known contradiction between peat core and flux derived C accumulation rate estimates (we did provide two references: Clay et al., 2010 and Ratcliffe et al., 2017). There is nothing "odd" about this, as we shall outline below. For example, for the flux method to be considered as robust, carbon fluxes need to be monitored across the entire burn rotation. Only then could one directly compare C balances between flux measurements and long-term peat core C stock assessments (which, we agree, should in principle be possible but in practical terms is hardly ever possible because of flux monitoring timescales being limited by funding, as in our case of only five years of Defra funding). However, there are additional issues with the flux approach and its application over short timescales. For example, there is substantial climatic variability over short timescales (variability rather than reflecting a long-term mean), and post-management vegetation regrowth leads to initially lower overall ecosystem respiration C losses (with predominantly young and as such photosynthetically active shoots) compared with older unmanaged stands of vegetation (with net photosynthesis in regrowing vegetation increasing rapidly to very high values before reducing again as older tissues build up, causing a higher respiration to photosynthesis ratio, until it reaches that of mature vegetation with much lower net C uptake rates; e.g., Gough et al., 2008). For our sites (as in most studies), only the net ecosystem exchange CO_2 fluxes for uncut (no management) plots could be compared to peat carbon stocks (Heinemeyer et al., 2019; forthcoming). Thus, the divergent results between flux and charcoal measurements are to be expected (because stocks accumulate over time as vegetation re-grows). Therefore, short-term fluxes cannot be directly related to peat core C accumulation rates as the vegetation at each site was of an unknown but fairly constant age (at an older growth stage). Any direct comparison to long-term peat core records would require long-term flux monitoring over at least an entire management cycle (i.e., ca. 25 years, including all major vegetation stages). Thus, long-term monitoring is required to accurately determine the biomass combustion loss versus charcoal input effect. Finally, for comparisons between peatlands within the context of climate change, we would expect not just CO_2 to be considered as part of "all internal C cycling," as stated by Evans et al., but at least also to include CH_4 measurements (decomposition fluxes from anoxic peat areas); the atmosphere sees both those C flux components (although further losses from dissolved and particulate carbon will also be of importance, depending on the peatland condition and topography). We propose that a robust flux monitoring approach for managed heather burning would require at least 25 years and should include all major C-flux components (i.e., Net Ecosystem Carbon Balance, NECB) and compare non-intervention with managed vegetation plots. This is precisely what our Peatland-ES-UK project (http://peatland-es-uk.york.ac.uk/10) is attempting to achieve, a challenging but we think urgently required research activity of considerable policy relevance.

Overall, we strongly dispute the claim made by Evans et al. that our interpretation of managed burn impacts on peat C accumulation is not robust. If anything, we feel it provides one of the most robust efforts to date (considering our above comments on the other two similar studies: Garnett et al., 2000 and Marrs et al., 2019). Moreover, our hypotheses did not require within site replication because replication was provided between sites (nevertheless, additional within site cores would have been collected if more funds were available). This enabled us to incorporate the full range of real-world climatic and site differences found across UK grouse moors, which should be seen as an advantage rather than a confounding factor, since consistent relationships were found across all three sites. Comparing our study to tropical peatlands is not appropriate; as Evans et al. rightly state: "Indonesian peatlands and UK blanket bog differ in many respects." While there are peat physical differences (peat structure and bulk density), UK grouse moor burns are predominantly "cool burns" compared with predominantly very hot fires in tropical forests, likely resulting in the actual peat catching fire (e.g., Boehm et al., 2001). Finally, decomposition in tropical systems is subject to much higher mean annual temperatures (UK upland bogs about 5°C, e.g., Garnett (1998), compared with tropical systems of about 26°C, e.g., Könönen et al., 2016), leading to faster microbial decomposition (Davidson & Janssens, 2006; Hirano et al., 2014). Additionally, we should not be basing "current understanding" about burning impacts on UK blanket bogs (or elsewhere) on using one site, Hard Hill, which is not truly representative of grouse moors across the UK (e.g., the burn rotation is too short for the very extreme wet and cold conditions). Clearly, we need to move beyond the Hard Hill plots – our study does precisely this and should therefore be welcomed for its vital contribution to the depauperate evidence base.

The final section on policy implications (e.g., Habitats Directive) in the light of the potential UK departure from the EU seems a very odd addition. It appears to overlook the limitations in all of the currently available studies: we do not currently know how burn management impacts C accumulation across the wider landscape scale (nearly all studies are plot based and dominated by the Hard Hill plots). We particularly lack data on how topography affects erosion and decomposition impacts. In our paper, we point out that there is a real danger of considerable C losses from steeper slopes (i.e., by burning exposing peat to water erosion). We do not want to be drawn into a general discussion around fire management, but the recently growing evidence (the major studies are cited by Evans et al.) highlights that previous assumptions based on one study are questionable, which is unsurprising. Science should be robust and evidence should be seen in light of this, and should also consider evidence objectively. The best and most exciting science often challenges our strongly held common perceptions. In particular, modelling studies, so far, do not represent any potential C accumulation from charcoal (e.g., Heinemeyer & Swindles, 2018), which should be considered based on our data and findings from other related studies, such as by Clay and Worrall (2011). Evans et al. also refer to greenhouse gas (GHG) emissions in relation to the Peat Strategy. We agree that evolving policies should be based on a robust and reliable scientific evidence base. However, the evidence on GHG emissions from UK blanket bogs, particularly under grouse moor management, is extremely limited (e.g., Harper et al., 2018). Rewetting large areas could potentially lead to large increases in methane emissions (see Heinemeyer & Swindles, 2018; Heinemeyer et al., 2010), particularly during warm and wet years (and future warming), as highlighted by Heinemeyer et al. (2019; forthcoming). However, a robust understanding of management impacts on GHG emissions requires long-term research and needs to examine evidence alongside other benefits, such as carbon storage and water quality (i.e., ecosystem multifunctionality). Another limitation of our current knowledge on burning impacts is that many studies to date have used a Space-for-Time (SfT) approach (such as in the Ember study, e.g., Brown et al., 2015) – a more robust Before-After-Comparison-Impact (BACI) approach (see Schwarz, 2014) requires more resources and time, but overcomes issues such as generic site differences which may influence results (site differences are often unknown or ignored in SfT studies). We believe that studies such as ours (i.e., Peatland-ES-UK; Heinemeyer et al., 2019; forthcoming) provide the long-term and robust evidence required to inform policy. For example, the Peatland-ES-UK study has now run for seven years and uses a BACI approach with catchment-scale and plot-level replication.

In conclusion, our defence clarifies the misunderstandings and misconceptions held by Evans et al. in relation to the scope and objectives of our study. We do, however, agree with Evans et al. that our findings have clear limitations. But we would also highlight that most of the criticisms made by Evans et al. are based on issues which we previously addressed in our paper (such as the lack of an unburnt control, other dating tools and wider catchment and site assessments). We would also argue that our study provides a vital addition to the prescribed burning evidence base, albeit in a very narrow context of UK grouse moor management on blanket bogs under specific climatic and environmental conditions. Our study will hopefully stimulate funding bodies to support further (and specifically long-term) work so that the many remaining research gaps can be addressed – this is vital if we are to implement environmentally sound and scientifically robust land-use policies.

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